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ART. CXCVIII.—*Researches in Embryogeny*. By W. WILSON, Esq.

(Continued from p. 735).

THE following is a summary of what has already been published in the 'London Journal of Botany' for December, and is here added as a necessary sequel to what has already appeared in 'The Phytologist.'

Since the time when Dr. Giraud's article on Tropæolum majus was partially noticed, I have made that plant the subject of close examination; and the result is that I am compelled to dissent from Dr. Giraud's account in several particulars. It is especially objectionable because (contrary to the evident design of the writer) it lends undue countenance to the theory of Schleiden, in the erroneous statement that there exists a traceable mass of "conducting tissue of the style into the carpellary cavity as far as the exostome." In researches like these, it is seldom that the positive statements of other observers can be absolutely disproved; but here the error admits of demonstration. Dr. Giraud says concerning the "suspensor," that its upper extremity protrudes "through the apex of the nucleus and the micropyle," and that from this extremity a number of cells "hang loosely in the passage leading to the conducting tissue of the style, while the rest unite in forming a process which passes down the outer side of the ovulum within the carpellary cavity." Here we have the position of the supposed passage distinctly marked, as being *above the micropyle;* whereas it will be seen that the supposed "loosely hanging cells" must belong to a particular portion of the "suspensor" which never protrudes beyond the micropyle in the manner that it is stated to do. The curious structure and development of this part has escaped the scrutiny no less of Dr. Giraud than of Schleiden and other observers. In the following remarks I shall adopt the term suspensor for the body extending upwards from the embryo, though I am not satisfied that it is quite in accordance with the original views of Mirbel thus to apply it. Schleiden, in his attempt to account for the manner in which the suspensor gains egress from the coats of the ovulum (for there are really two of these surrounding the nucleus), invents a spurious theory, and gravely asserts that these coats become "obliterated" or undergo "resorption;" and thus the apex of the suspensor is laid bare; whereupon it assumes the form represented at fig. 3; the part *a* being directed

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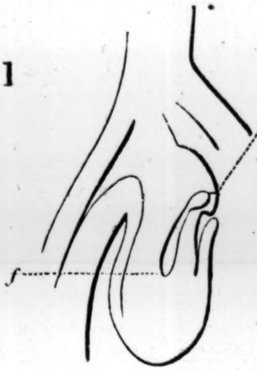

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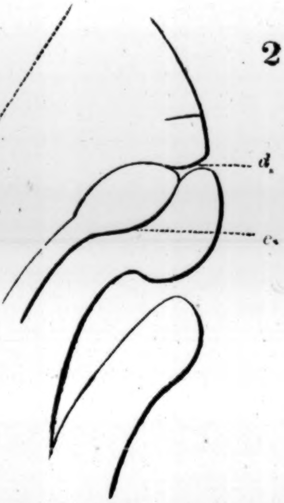

Fig. 1.—Longitudinal section of ovulum from a flower of *Tropæolum majus* previous to fecundation, with the principal part of the carpellary integument removed, showing the micropyle *d*, and the embryo-sac *f*.

Fig. 2.—A portion of the same more highly magnified, showing the micropyle *d*, and the primary utricle *e*.

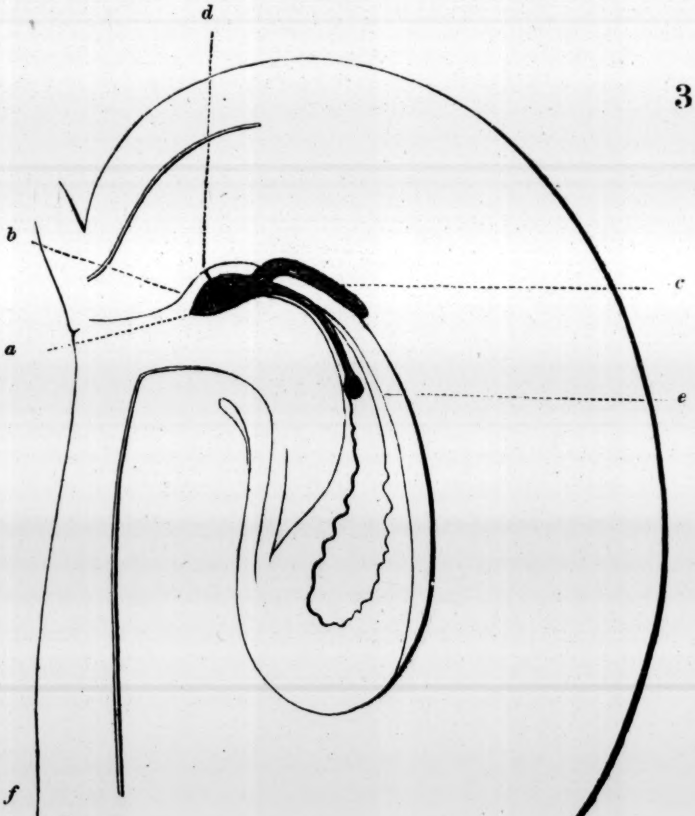

Fig. 3.—Longitudinal section of unripe seed soon after the protrusion of the suspensor; *c*, the posterior process; *a* the anterior process of the suspensor; *b* the swelled knob connecting both processes with the lower portion of the suspensor; *e* the embryo in a very early stage; *d* the micropyle; *f* the pore to which the process *a* extends at the period of maturity.

towards the axis of the flower. Dr. Giraud, on the other hand, as positively asserts that the suspensor passes out at the micropyle, through which "it may be drawn by a slight traction completely out of the nucleus, along with the suspended embryo." Both of these accounts are quite at variance with the fact. The swelled knob, *b*, never extends beyond the micropyle, but is in every stage included within the coats of the nucleus, and from this knob are sent forth two filamentous processes, of which only the posterior one (*c*) has hitherto been noticed. This process does not pass out at the micropyle, but perforates the coats of the ovule immediately below it on the outer side.

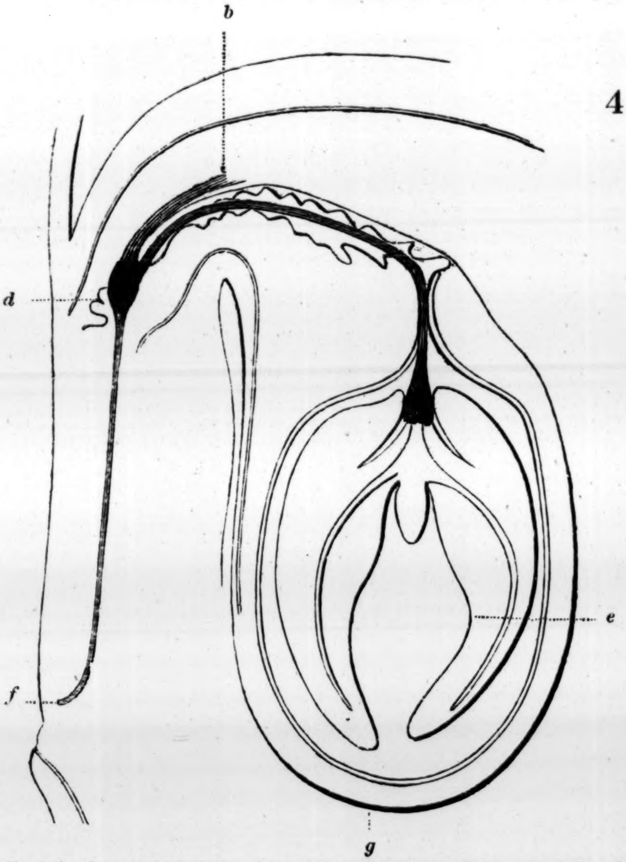

Fig. 4.—Longitudinal section of the ripe seed:—*d* the micropyle; *f* the extremity of the anterior process of the suspensor. The posterior process is continued from *b* to *g*; *e* the embryo.

The anterior process (*a*) first pushes its way through the substance of the neck of the ovule; then through what may be considered the placental tissue. It then reaches, or forms for itself, in the substance of the carpellum, at its inner angle, a canal which lies parallel with and very near to the common axis of the three carpella; and passing along this canal the process arrives at the lowest point of attachment of the carpellum with the receptacle where there exists a small pore. The carpellary integument would thus seem to constitute an essential part of the seed, and its removal before sowing would most likely prevent the seed from germinating; for it seems reasonable to conclude that the two processes of the suspensor exercise the function of radicles; and at least one of these would be broken off in the act of removing the carpellary coat. It is a curious circumstance that the ovulum in this genus has no free funiculus, and that its coats forming the primine and secundine are so blended with the rest of the tissue which connects the ovulum with the carpellum, that no positive line of separation can be detected in that part. Indeed it is only by an attentive examination of the micropyle that the existence of two coats can be clearly ascertained.

It is proper that I should notice here another somewhat mistaken view entertained by Dr. Giraud, who states that there exists before impregnation "a small elliptical cavity near the apex of the nucleus, having a delicate lining membrane formed by the walls of the surrounding cells: this cavity is the embryo-sac, and a minute canal may be traced leading from it to the exostome." It would be more appropriate to say that the *membranous lining* of the cavity constitutes the embryo-sac. The cavity, however, is not confined to the apex of the nucleus: its form and dimensions are represented at fig. 1, which exhibits a longitudinal section of the ovule and the contained nucleus. If the lining of this cavity be, as I suppose, the embryo-sac, the contained embryo with its suspensor must have a different relation to it from what has been hitherto advanced by either Schleiden or Mirbel; for about the period of fecundation the embryo forms a small elliptical body, very much smaller than the cavity in which it is lodged; it is indeed so minute as to be with great difficulty displayed under the microscope. Its position and relative size are dimly seen at *d*, fig. 1, and more clearly represented in the more highly magnified fig. 2. This rudimentary embryo is described in Dr. Giraud's paper as "a quantity of organizable mucilage enclosed in the embryo-sac." It is true that the whole of the nuclear cavity is lined with a lax delicate membrane, easily separable from the nucleus itself; and that at the time when the suspensor is about to perforate the coats of the ovule, this membrane may be traced from the apex of the suspensor to the bottom of the cavity, clearly disproving what Schleiden has asserted concerning the introversion of the embryo-sac by the intrusion of a pollen-tube.

I am at a loss to understand in what mode Dr. Giraud has been able to identify "the fovilla and its granules," which he records as having been found abundantly in the passage leading from the style to the exostome, at the time indicated by his "fourth period." If the process *a* of fig. 2 be regarded as a kind of finger-post, it would lead to the conclusion that he has mistaken for fovilla the particles of amylon which are found in abundance in the neck of the ovulum.

Warrington, January 12, 1844. W. WILSON.

ART. CXCIX. — *Reply to Mr. Edmonston's 'Remarks on Botanical Classification.'* By PH. B. AYRES, Esq., M.D.

Thame, Oxon, January 20th, 1844.

SIR,

I have waited, until now, with the expectation that some more able correspondent of your journal would have replied to the remarks of Mr. Edmonston on the natural arrangement of plants; but as no one has come forward, I shall endeavour to answer his objections to that system.

With his first propositions, "that the present system of entirely dispensing with an introductory or artificial scheme, is not only wrong in theory and opposed to the principles of philosophical classification, but almost impracticable in practice;" and, "that a simple artificial scheme is absolutely necessary as an easy introduction to the study of the science, by which a sufficient knowledge of species may be gained to enable the student to turn to the more complicated generalizations of the other system;" — I in part agree. That an artificial scheme is necessary to an *easy* acquirement of the science, is tantamount to saying that an alphabetical arrangement is necessary to the finding easily any particular subject in a bulky Encyclopœdia, or a good index to any other work of reference; and such is the artificial system of Linnæus to the vegetable kingdom. The assertion of the absolute impracticability of learning Botany without the assistance of an artificial scheme is absurd; inasmuch as species may be discovered by the natural system, although, I acknowledge, with greater difficulty. It is easier, for example, to find a species in any work on descriptive Botany, provided the name be known, by turning to the index, than to wade through the descriptions of classes, orders and genera to arrive at it. Exactly in the same manner is it easier to turn to the Linnæan index, than to wade through the long analyses of natural orders given in Lindley's 'Introduction to the Natural System.' With similar restrictions we may acquiesce in the third proposition, "that what was then (in the time of Linnæus) necessary for the state of botanical knowledge generally, is, I contend, still indispensible to students individually;" that is, a good index of plants!

From the next proposition I must entirely dissent; namely, that "Nature creates species;" for it is easy to perceive that the term species is applied to an assemblage of individuals, which, from their great resemblance to each other, and from their capability of propagating their like, are called *species*. Were the *creation* of *species* allowed, I cannot see any rational objection to the admission that genera, orders, classes, &c. were created; for what is true of a smaller assemblage may be true of a larger one. On the other hand, I contend that Nature simply created individuals capable of propagating their kind; and if we trust to the Mosaic account of the creation, we shall see reason to believe that plants as well as animals were individually created. It is evident that the term species applies to one of the primary inductions from a comparison of individuals; not that a species has any absolute existence, but is merely the conception or expression of the points of resemblance of a certain number of individuals bearing the greatest resemblance to each other. Another argument against Mr. Edmonston's view, is the fact of the existence of varieties; a still lower induction from individuals!

Two other propositions are contained in the same paragraph of Mr. Edmonston's letter. 1. That it is difficult to establish the hypothesis that Nature has created plants upon a certain plan, and thrown them into classes, orders, genera and species: and—2. That this scheme of Nature is identical with one or other of the natural systems.

I cannot discover the difficulty that Mr. Edmonston conjures up against the proof that Nature has followed a plan in the creation of plants; for the fact that a plan has been followed in their creation, is established by the simple fact, that plants are capable of being arranged and distributed into classes, orders, genera and species; for had they been created at random, they would form a chaotic mass, and no such regular gradation of affinities could have been established. The very fact that one great group of plants possesses distinct sexual organs, while another is destitute of them; and that the individuals belonging to these two great divisions possess many other points in common (as for example the presence of vessels in the sexual group), and that these are connected by certain oscillatory groups; is sufficient evidence of a plan in the creation of plants. Unless a plan were followed, all attempts at classification would be futile.

By what I have said above I do not mean to infer that what we term classes, orders, genera and species were formed by Nature, for that would give these abstractions real existence; they are indeed nothing more than creations of the mind of man; they are the expression of the resemblances of plants one to another in a series of abstractions or inductions. We cannot then say of any of our arrangements of plants, that they are natural in the sense that they have been created by Nature; but that they are natural in the sense that they bring together those plants that have the strongest analogies.

Now this brings me to the second proposition, "that this scheme (or plan) of Nature's is the same as one or other of the natural systems." Supposing it has been proved above that Nature has followed a plan in the creation of plants, and that consequently plants have such analogies to each other as to enable us to arrange them in species, genera, orders and classes; it is evident that that system will approach nearest to the plan of Nature, which exhibits the greatest number of analogies in its construction, and that brings together in its various groups those plants that bear the greatest resemblance to each other. Your correspondent asks whether any one of the natural systems corresponds or is identical with the scheme of Nature? He will perhaps think himself triumphant when I answer, *No.* But his triumph will be short when I in return ask him the following questions. Is our knowledge of Nature perfect, and are the general theories of any one of the natural sciences incapable of change? Does not the history of each science (save the Mathematical) show a series of changes in its general propositions? Has not all Natural Philosophy undergone great changes, and is it impossible that it should hereafter undergo still greater changes? If then all sciences have been and still are liable to change; why should not our science undergo such changes? But I shall be glad to be informed what botanists have considered any form of the natural system as a settled and perfect system; and who does not expect changes in system contemporaneous with the increase of our knowledge of the vegetable world? A botanist who thinks that *all* has been done, that the science is perfect, and should undergo no further alterations, I should think a fit inmate for a lunatic asylum!

The quotation from Dr. Lindley's 'Key to Botany' is not sufficiently ample; Mr. Edmonston should have added the next paragraph, when it would read thus: — "What we call the characters of plants are merely the signs by which we judge of affinity, and all the groups into which plants are thrown, are in *one* sense artificial, inasmuch as Nature recognizes no such groups. Nevertheless, consisting in all cases of species very closely allied to each other, they are in another sense natural." The addition of the last sentence very much alters our notion of Dr. Lindley's expression; for now he implies that Nature has not indeed created species, orders, genera, or other groups as such; but has imprinted such characters and affinities on plants, as enable us to throw them into groups approximating more or less to the scheme of Nature, and in that sense natural.

Mr. Edmonston's remarks on the process by which botanical knowledge is or may be most easily acquired, are for the most part good; still I cannot help considering him mistaken on some points. For example, he says that a finished botanist has no need of classification; and here I differ from him. I grant that a botanist who knows all the British plants, does not need classification for the purpose of distinguishing species, since he knows them and their characters; but still he needs it to show their analogies and affinities: and I contend that were a man capable of comprehending in one view the whole vegetable kingdom, with all the distinctions and analogies of species, the natural tendency of his mind would compel him to throw them into groups; and supposing his knowledge perfect, he would construct a perfectly natural system. Hence the natural system will become more and more perfect by its progressive changes, and approximate more and more to a conformity with Nature. It appears to me that Mr. Edmonston, throughout his letter, lays more stress on the distinguishing of species from each other, than combining them into a coherent whole. He would consider Botany as an analytic rather than a synthetic science, or both combined.

Descending from the high ground we have hitherto occupied, I must be permitted to notice a few of the detailed objections put forward by Mr. Edmonston against the natural system. He says that there has never been a system which was not liable to exceptions, yet he expects that the natural system should be liable to *none:* and commencing with the three primary groups — Mono- Di- and Acotyledons, he discovers a few exceptions, and these he may be permitted to make the best of. But what would Mr. Edmonston say, if the same species (Nature only creates species) should be found usually with two, but occasionally with three or four cotyledons? Would he separate the individuals and place them in the two primary divisions, although in other respects they perfectly resembled each other? Yet this would be necessary to produce his idea of uniformity. Such a circumstance has been observed in Sinapis ramosa, and is figured in Decandolle's 'Organographie,' plate 53. It is true that Acotyledonous plants may be found among Dicotyledons; indeed we should not expect two seed-leaves in a plant entirely destitute of leaves, like the Cuscuta, although in all other respects it may be and is accordant with our idea of Dicotyledonous structure. But if Mr. Edmonston will cast his eye over the following table, he will perceive that the individuals of these primary groups are distinguished in all parts of their structure.

ROOT.	STEM.	LEAVES.	PRTS. OF FLOWER.	SEED.
Endorhizous.	Endogenous.	Reticulate.	Ternary.	Monocotyled.
Exorhizous.	Exogenous.	Parallel Veins.	Quaternary or Quinary.	Dicotyledonous.
Heterorhizous.	Acrogenous.	Forked Veins.	Absent.	Acotyledonous.

Here the Monocotyledons are Endorhizous, Endogenous, with parallel-veined leaves, and a ternary division of the flowers; while the Dicotyledons are Exorhizous, Exogenous, with reticulate-veined leaves, and a quaternary or quinary division of the flower. Here is a set of characters which will clearly distinguish a Monocotyledonous from a Dicotyledonous plant, although some one or other of the characters may be departed from in a particular instance. Moreover, Mr. Edmonston forgets that the definition of the Dicotyledonous group includes those plants that have two or more *opposite* cotyledons; while that of Monocotyledons admits plants with two cotyledons, if they are *alternate*. Lindley, in his definition of Dicotyledons says, "Embryo with *two or more opposite* cotyledons;" and in that of Endogenæ or Monocotyledons, "Embryo with but one cotyledon, or if with two, then the accessory one imperfect and alternate with the other." After this explanation, the objection to the terms Mono- and Dicotyledons becomes a mere verbal quibble.

What is said above will also answer another objection of Mr. Edmonston's, "that the structure of the seed is too much used in the definitions of the natural school," and "that the characters of the primary divisions ought always, if practicable, to be taken from parts easily seen, or at least not requiring such delicate microscopical investigation as the natural system requires;" for if he finds a plant whose germination is exorhizous, whose stem is exogenous, whose leaves have reticulated veins, and whose floral organs are quaternary or quinary, he may be sure that it is a dicotyledon without an examination of the seed; nay, if he only observe that the stem is exogenous, the leaves reticulated and the parts of the flower quaternary or quinary, he will never be mistaken as to the group to which he ought to refer it; and these parts are, I imagine, sufficiently obvious.

But the examination of the structure of the seeds of plants is moreover absolutely necessary in any system where the affinities of plants are taken into account. Linnæus used the parts of the flower as the basis of his artificial system, as being the least variable of the parts of plants; and it must be allowed that the structure of the seed is even less variable than that of the other parts of the flower. I can assure Mr. Edmonston from personal experience, that the dissection of seeds is by no means so difficult as he considers it; that a little practice will enable him to dissect most seeds; and that, except in very minute ones, a common pocket lens will be sufficient for his purpose. I grant that some patience is requisite to do this well.

We now arrive at a very grave charge against the natural system — the indefiniteness of its groups; and Mr. Edmonston has chosen Ranunculaceæ as the subject of his animadversions, finding no character so definite as the one stamen of Linnæus's Monandria. Now it is curious that the very indefinite characters of Ranunculaceæ may be included in a definition to which scarcely any (certainly not more than in the Linnæan classes) exceptions can be found. I give this definition from Lindley's 'Introduction to the Natural System,' p. 6. "Polypetalous dicotyledons with hypogynous stamens, anthers bursting by long slits, several distinct simple carpella, exstipulate leaves sheathing at the base, solid albumen and seeds without arillus." Lindley gives the following exceptions or anomalies. "In Garidella and Nigella the carpella cohere more or less. In Thalictrum, some species of Clematis and some other genera, there are no petals. Pæonia has a persistent calyx." Now does it not strike Mr. Edmonston as exceedingly improper that plants possessing a corolla should be placed in the same Linnæan genus with those in which it is totally wanting!! We must surely complain of the genus before we find fault with the order. I would suggest the revision of the genus to Mr. Edmonston.

In Garidella and Nigella it is true that the carpels are more or less coherent. I do not happen to have access to detailed descriptions of any species of these genera; but, if I am not mistaken, the degree of cohesion of the carpels varies very greatly in different species, so that in some they are almost distinct. In Actæa I find that the carpella are reduced to one, and that one has taken a baccate form, exactly as some Leguminosæ are drupaceous.

Mr. Edmonston has unfortnnately fallen into the error of taking the detailed descriptions of the natural orders for their distinctive character; and he has found in the former that degree of indefiniteness which might be expected in giving a detailed account of the structure of a large group of plants. Mr. Edmonston, in criticising some expressions of Dr. Lindley, with reference to the two great systems, says,— "Now if it be the case that the student must go through the same process — examine the same parts — in the one as in the other system, it will be singular if the amount of knowledge is not equal. In fact the examination necessary for the knowing the genus and species of a plant, after you have got at its class and order by the Linnæan system, gives as great an amount of information concerning the plant itself, as if this end were attained by the natural system, with this difference, that the information is far more easily acquired and the process not nearly so complicated."

Here I submit that Mr. Edmonston labours under a very grave error, from his want of appreciation of the objects of the natural system. It is true that the student may gain an equal knowledge of the plant itself as by the natural system; but when he has obtained that knowledge, what has he discovered of the relations of the plant to other plants, farther than the generic group. When he rises above that group, and comes to the orders and classes, what does he find but "combinations of disjointed things"—genera associated that disagree in every particular, excepting that they have the same number or arrangement of stamina and pistils, and even then, there are numerous exceptions to these numbers and arrangements!! If we consider plants in their affinities or properties, we shall find that the Linnæan groups are, for the most part, forced and unnatural, while the reverse is usually the case with the natural orders. In saying this, I do not mean to deny that there are numerous exceptions to the natural system, but as that system can never be perfect until the whole vegetable kingdom is known, these exceptions must still exist while man is less than omniscient!

I will conclude with, I fear, a rather long extract from Swainson's admirable treatise 'On the Geography and Classification of Animals,' which sets forth, in better language and more forcible manner than I am able to employ, the differences and uses of the two systems; at the same time remarking that Linnæus himself could not rest contented with his own admirable artificial scheme, but sketched out a plan for a natural one!

> "What, then, is the difference between an artificial and a natural system? The first is for the ready discrimination of species; the latter for the elucidation of those resemblances which such species bear to others in all their varied and complex relations. The one stops where the other begins. We make use of an artificial system to become acquainted with the name of a species; and to learn all that has been written on its peculiar structure. We turn to the natural system, to know the probable station of this species in the scale of being, the affinities it possesses to others, and the analogies by which it is related and represented. Hence the perfection, as we have frequently intimated, consists in the clearness and precision of its subdivisions, and the facilities which it affords to determine the name of the object we are in search of. In this respect a good artificial system is to be judged by the same rules as those by which

we should decide on the merits of a copious index to a voluminous publication, for the purposes of both are the same: both are equally useful, and the merit of both lies in clearly directing the reader to the precise point on which he desires information. A good artificial system is, therefore, not only a useful, but even, in some respects, a valuable invention, requiring much more skill than is generally supposed; and it is, perhaps, much more adapted for general use than any other. The most admirable classification of this sort ever invented, is that denominated the sexual system of plants, by Linnæus. Many natural assemblages are preserved without any great violation of the principles on which he set out. This is always a great recommendation to an artificial system, yet it is by no means necessary to its formation. Natural affinities may be overlooked wherever they interfere with precision of arrangement, the first are secondary, the latter primary. We open an artificial system to come to the knowledge of a matter of fact; but if we wish to proceed farther, and to know how this fact bears upon *other* facts, we turn to the natural system. Such are the uses of the two methods of classification upon which we have been speaking, and such the theoretic distinctions by which they are separated. Between them, however, is a third sort of system, which, from combining artificial division with some regard to natural affinities, are generally termed mixed systems, or half-artificial methods, while others (and generally among this number are the authors themselves), have pronounced them *natural* arrangements."

I fear, Mr. Editor, that I have quite exhausted your patience, and shall now subscribe myself, Your obedient servant,

Ph. B. Ayres, M.D.

To the Editor of 'The Phytologist.'

Art. CC.—*An Account of a Visit to Teesdale in the Summer of* 1843.

By James Backhouse and James Backhouse, jun.*

As an account of a visit made last summer by my father and myself to Teesdale and some adjacent parts, may interest the readers of 'The Phytologist,' I send thee some account of it.

We set out on the 28th of 8th month, and proceeded by railway to Darlington; from thence we went to Bishop Auckland, where we left the railway, and walked to Shull between Hamsterly and Wolsingham. There we found Hieracium boreale, Scutellaria minor, Listera cordata, Vaccinium Vitis-Idæa, Pyrola media and minor, Lastræa Oreopteris, Polypodium Phegopteris and Dryopteris, and Equisetum sylvaticum. In the native birch-woods at this place, the broad and narrow leaved varieties of Convallaria majalis occur; and a robust, upright and very hairy variety of Veronica scutellata? is found in ponds. Dianthus Armeria occasionally springs up here, where the ling has been fresh burned off. The day following we walked over the moors, by compass, to the High-force inn, Teesdale, a distance of about seventeen miles. We met with Vaccinium Oxycoccos and Empetrum nigrum, especially the latter, in many places. In a swampy place facing the south, about two miles from Shull, we found Malaxis paludosa in full flower. Probably the reason of many persons not finding this plant, when searching for it in known localities, is, that they seek it too early in the year. We also met with Rubus chamæmorus in several places. When we got within sight of the High-force inn, we descended into the valley of the Tees, near Winch-bridge, where we found Hieracium rigidum, diaphanum, umbellatum? boreale and Lapeyrousii, Crepis succisæfolia, Galium boreale, Thlaspi alpestre, Pyrus Aria, Melampyrum sylvaticum, Trollius europæus and Equisetum variegatum. We visited also the High-force waterfall that evening, near which we found the recently described Poa Parnellii, and saw abundance of Potentilla fruticosa, several Hieraciums and Cnicus heterophyllus. During most of the day grouse and curlews were to be seen on the moors.

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* Communicated by James Backhouse, jun.

After a comfortable night's rest at the High-force inn, where there is good accommodation, we set out in the direction of Micklefell: we crossed the low end of Widdy-bank, which is the locality of Carex capillaris, Eriophorum pubescens, Bartsia alpina, Tofieldia palustris, Polygonum viviparum, Gentiana verna, Gymnadenia albida, Saxifraga aizoides &c., all of which we had found the year previous. We then ascended the face of Cronckley-fell, where were Allosorus crispus, Epilobium angustifolium, Arbutus Uva-ursi, Hieracium Lapeyrousii, pulmonarium &c. Leaving the locality of Dryas octopetala and Juncus triglumis on our right, we crossed the top of the fell, where we found Draba incana, Saxifraga hypnoides, Helianthemum canum, Hippocrepis comosa, Lycopodium alpinum and Selago and Gentiana verna, the latter in great profusion. Here we saw a large flock of grouse—from thirty to forty. We next ascended Mickle-fell, which is the highest mountain in Yorkshire, being 2,600 feet above the level of the sea. Saxifraga stellaris, Sedum villosum and Cochlearia grœnlandica were growing in the streamlets which descend from it. The top of Mickle-fell is limestone covered with grass, thickly interspersed with Gentiana verna. In the margins of the springs which emerge from under this limestone, Epilobium alsinifolium grows in abundance. Descending the west side of this mountain, we crossed Maizebeck and the Tees, below Cauldron-snout; near the junction of these rivers is a spring, filled with Saxifraga stellaris. We found three small plants of Woodsia Ilvensis on Falcon-clints. A large bird of prey was sailing round the tops of these crags, which we thought was an eagle, but it did not come near enough to enable us to determine this with certainty. We then ascended the rocks by Cauldron-snout, and returned to our quarters across the back of Widdy-bank-fell, where, in a stream from under the limestone, we found Juncus triglumis, Tofieldia palustris and Kobresia caricina. The sides of this stream were adorned with Bartsia alpina, Primula farinosa and Saxifraga aizoides, down to its junction with the Tees. Festuca vivipara, Sesleria cærulea, Thalictrum alpinum, Gnaphalium dioicum, and several other interesting plants, also grow near Cauldron-snout. The year previous we found several large tufts of Polystichum Lonchitis on Falcon-clints. Cystopteris fragilis and dentata, Asplenium viride and Trichomanes, grow there abundantly, together with a profusion of seedling ferns, some of which, at first sight, might be taken for Woodsia Ilvensis. There we have also met with a deeply incised variety of Asplenium Trichomanes, but we never saw anything of Asplenium fontanum.

The next day, accompanied by our friend Jacob Allison of Cotherstone, we again crossed the back of Widdy-bank-fell, passed the head of the Weel, and crossed Crook-bourn into Cumberland; we then crossed the Tees into Westmoreland, and traversed the moors in the direction of Dun-fell. When in a straight line between the Greenburn lead-mine shaft and Dun-fell hush, we came upon the new locality of Saxifraga Hirculus (Phytol. 741), which was plentiful, but sparingly in flower. In a limestone hollow near this place, Asplenium viride was growing in profusion in the crevices of the rock, and at the bottom, among nettles, we found some large plants of Polemonium cæruleum in flower; this was new to the Teesdale district. From this place we crossed over the back of Meldon-fell to Highcup-scar,* an enormous ravine, about a mile and a half long, and 1000 to 1500 feet deep, margined by basaltic cliffs, surmounted on each side by limestone, topped with sandstone crags. After having walked about twenty miles, mostly over bleak barren moors, without a tree and with scarcely a trace of cultivation, the contrast of the scene at this place was very striking, as this great gulf opened to our view the rich fertile vales of the cultivated part of Westmoreland. Sedum Rhodiola and Thlaspi alpestre were growing in abundance in the clefts of the basalt, and in a deep gorge we had the pleasure of finding several very fine specimens of Saxifraga nivalis, which I believe had not previously been found in England. We then returned towards the High-force inn, passing Maize-beck scars, where Potentilla Salisburgensis was abundant, and reached our quarters some time after dark, having extended our walk to about thirty miles.

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* Highcup-scar is generally named Eagle's chair in maps.

The day following, taking Winch-bridge in our way, where we found Potentilla Salisburgensis (alpestris) and other plants, we ascended Holwick-scars, and crossed over the moors to the old locality of Saxifraga Hirculus (which we found in abundance) near the junction of the river Baulder and the Black-beck. We stopped at Cotherstone that night and the next day, and the day following rode by way of Bowes and Brough to Appleby. Near Brough we ascended Hellbeck-scars, which are limestone, where I found Pupa Juniperi (a land shell) in great abundance. This is the only locality with which I am acquainted in the north of England. Here we also found Epipactis ovalis, Draba incana, and a large-leaved variety of Hieracium murorum. The following morning, passing Dufton-pike, a curious conical mountain near the village of Dufton, we again examined Highcup-scar, but did not find anything fresh worthy of notice, except Saxifraga nivalis in another basaltic gorge.

The day being clear we had a beautiful view of the Cumberland mountains, and in the distance those of Dumfriesshire. That evening we returned to Brough on our way home.

JAMES BACKHOUSE, JUN.

York, 12th Month 22, 1843.

ART. CCI. — *Notes on Carex teretiuscula, C. paradoxa, C. paniculata, and Mr. Gibson's C. pseudo-paradoxa.* By GEORGE LUXFORD, A.L.S., &c.

SINCE the publication of my brief note on these plants in the December number (Phytol. 811), so many opposite opinions have been expressed with regard to the one whose specific identity and relations are more particularly the objects in dispute, and these opinions have emanated from botanists of such high authority, that I must confess I feel considerable reluctance to publish any further observations on the subject; and it is only in redemption of an express promise, and the belief that no harm can possibly arise from a continuance of the discussion, that I now venture to print the following remarks.

Before I begin, however, I may perhaps be allowed to give a short history of my acquaintance with the plant from the neighbourhood of Manchester, which has given rise to the enquiry; and this I am the more anxious to do, since the opinions which I then gave were hastily conceived, as hastily expressed, and drawn from the examination of an insufficient number of specimens. I think it right to mention this, because there is a possibility that in more than one instance the opinions at that time privately given, may be found to be at variance with those subsequently formed.

Last summer I received from Mr. Sidebotham, of Manchester, specimens of some Carices on which he requested I would give him my opinion. At that time I happened to be much engaged, and was consequently unable to bestow on the specimens the attention which the subject required. Among them were examples of the disputed Carex from "Seamon's moss-pits;"* and these I could refer to no other species than Carex teretiuscula,† believing it to be that plant with its habit somewhat changed by local or other circumstances, in consequence of which the inflorescence had assumed a more elongate form than usual. Believing this, I was somewhat surprised to learn from Mr. Sidebotham, that some of his botanical friends, as well as himself, considered the plant to be the C. paradoxa (*Willd.*), which had then been recently introduced to the notice of botanists in Mr. Babington's Manual, as an established British species, found in Ireland. After strenuously endeavouring to discover a correspondence between the Manchester specimens and the description of C. paradoxa as given in the Manual, I began to think that the difficulty lay with myself solely, and that none was presented to others possessing better information on the subject. This led me, though reluctantly, to acquiesce in the opinion that it might be Carex paradoxa. Soon after this I was still more surprised to learn from Mr. Gibson, that the plant from Manchester was identical with the one to which he alludes as having "its fruit agreeing with Leighton's figure of the fruit of C. paniculata" (Phytol. 366), since I was not then aware that Mr. Gibson restricted the use of the term *fruit* to the *nut only*, and I had always applied the term to the perigynium and its included nut.

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* I believe that this locality has not previously been given in 'The Phytologist,' by any of the botanists who have written on the subject. I now mention it without hesitation, since, from the circumstance of the locality being expressly stated on the labels accompanying all the specimens of the plant that have come under my notice, it appears to me that there has been no attempt to make anything like a secret of the matter.

† Certainly not to *C. paniculata*, as by an extract from one of my letters to Mr. Sidebotham, it appears I once wrote. That this was merely a *lapsus calami* is evident, I think, from the context. I cannot plead guilty to having ever considered this plant a variety of C. paniculata.

At a subsequent period I was much gratified by receiving, in a packet of plants which Mr. Sidebotham had the kindness to send me, some additional specimen of Carex teretiuscula, and of the plant which even then had led to considerable discussion. The receipt of these specimens, which were in fine condition, again brought the subject under my consideration; and a renewed examination almost confirmed my first opinion, that it could be no other than a slight variety of Carex teretiuscula. Then came Mr. Gibson's note on this plant under the name of C. pseudo-paradoxa, (Phytol. 778). In that note Mr. G. has mentioned the relationship of the plant with C. paradoxa (*Willd.*), and at the same time correctly pointed out its distinctness from that species. This, it will be remembered, was followed by Dr. Wood's note (Phytol. 809), wherein are minutely described the peculiar circumstances attending the growth of the plant, and which circumstances, in Dr. Wood's opinion, are sufficient to account for the various discrepancies between this plant and C. teretiuscula, which he considers the normal form. The very beautiful specimens of the two plants obligingly forwarded by Dr. Wood, together with others, equally beautiful, of C. paniculata, in addition to specimens of all the three previously in my possession, formed a valuable store of materials, both for examination and comparison. The note appended to Dr. Wood's communication (Phytol. 811) contained the result of a careful examination of the materials then at my command; and I regret that I have since been prevented from recurring to the subject. Want of space even now compels me to defer the conclusion of my notes to another number; when the remarks I may have to make on these Carices, will be illustrated by figures, which have been most obligingly placed at my disposal by Mr. Wilson. I may in this place, however, be allowed to state, that my opinion as to the specific identity of C. teretiuscula and Mr. Gibson's C. pseudo-paradoxa remains unshaken, nay, it is rather strengthened than otherwise by recent investigations.

Before I conclude I would express my thanks to Mr. R. Spruce of York, for beautiful specimens of Carex paradoxa, from Heslington fields, near that city; and to Mr. Wilson and the Botanical Society of London, for the loan of others collected at Westmeath, Ireland, by Mr. D. Moore. G. Luxford.

2, Ebenezer Row, Kennington Lane,
February, 1844.

(To be continued).

Art. CCII.—*Two Botanical Visits to the Reeky Linn and Den of Airley, in April and June,* 1842.* By William Gardiner, jun. Esq.

My first visit to the Reeky Linn was on the 13th of April. I left Dundee with the earliest railway train for Newtyle, and from thence walked the remainder of the way, crossing the hills behind Alyth, and coming down upon the Isla at the bridge of Craig. The only plant of interest noticed in my route was Bryum albicans, which grew abundantly on a wet bank between Newtyle and Meigle. In the same locality I found it again plentifully in May, with ripe capsules, and associated with Dicranum varium. I took up my abode in Mrs. Robertson's "public," which is conveniently situated within a quarter of a mile of the Linn, and after dinner went out to botanize. The Reeky Linn is a beautiful and picturesque waterfall on the Isla, at the head of the Den of Airly, and about four miles above the castle. The water takes three distinct leaps, and throughout its progress foams like a boiling cauldron, and meeting at the base of the rocks with a jutting cliff which obstructs its fury, a quantity of it is dissipated in vapour, which, rising in the air, seems like a mist or smoke (*Scotticè* "reek"), hence the name that has been bestowed upon the fall. The vicinity of such a place, from abounding in moist rocks, is always rich in vegetation, and here, though too early for most flowers, I reaped a luxuriant cryptogamic harvest. Sticta sylvatica covered the rocks in many places, and large patches of the elegant Jungermannia pubescens were intermingled with J. platyphylla and var. *β. major*, Hypnum myosuroides, commutatum and prælongum, Anomodon viticulosum and curtipendulum, Bartramia Halleriana and pomiformis, var. *β. major*, and the lovely Neckera crispa. Here and there tufts of Didymodon Bruntoni showed themselves profusely in fructification, and, peeping from among them, the delicate calyptræ and bright tawny capsules of Encalypta ciliata, and the graceful little Hypnum pulchellum. The beautiful Hymenophyllum Wilsoni was here, as elsewhere, associated with Jungermannia spinulosa. On the banks, chiefly growing on sandy deposits, were Bryum punctatum, rostratum, marginatum, turbinatum, ventricosum and hornum, Hypnum palustre and uncinatum, Trichostomum aciculare, Dicranum flavescens and adiantoides, Orthotrichum rupincola and Didymodon rigidulus. Still closer to the water, in shady nooks, dwelt Marchantia (Fegatella) conica and hemisphærica and Jungermannia epiphylla; and, bathing in its liquid freshness, floated the dark branches of Cinclidotus fontinaloides, and Grimmia rivularis, (*Brid.*). Tufts of the elegant Didymodon capillaceus looked out here and there from the crevices of the rocks, with, more sparingly, Bryum androgynum and Hedwigia æstiva. Some of the rocks were encrusted with Lecanora tartarea; Squamaria hypnorum appeared in several places, and various Collemæ, particularly C. nigrescens and lacerum, attached themselves to the exuberant mosses. The trees were adorned with a profusion of Orthotrichum crispum, Jungermannia complanata and dilatata, Ramalina fastigiata and fraxinea. On the moist rocky banks that margined the stream, various Jungermanniæ were abundant, and I culled, among others, J. Lyoni, furcata, excisa, multifida, albicans, nemorosa, undulata, and, more interesting than all, J. Blasia *in fructification*, a rarity amply rewarding my day's exertions.

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* Read before the Botanical Society of Edinburgh. Communicated by Mr. Gardiner.

I next morning breakfasted early, and hastened again to the Linn. Crossing the Isla by the bridge of Craig I went down the north side of the stream, and viewed the waterfall from a variety of points, from all of which the eye feasted on pictures of varied grandeur. The shadows of night had departed, and all was now distinct. The sunlight sparkled on the foamy Linn, and seemed to penetrate the deep pool beneath, while the wild cavern on the southern side opened its dark jaws, and displayed a confused mass of the wrecks of fence and woodland which it had gorged in flood and storm. In the upper air now floated the lark's sweet melody, and the thrush, from the yet almost leafless trees poured its matin song. Wood anemonies and primroses were profusely scattered over the rocky and wooded banks; and the thickly springing verdure on every side gave promise of a summer luxuriance of vegetation. And as I thus revelled in the beauty and magnificence around me, and thought of the bright and sunny future, I felt with the poet, that—

> "From Nature and her changes flow
> An everlasting tide of joy."

My walk down the den of Airly, from there being no regular footpath, was rather fatiguing, but the rich succession of wild and rugged scenery, varying at every step, proved a more than sufficient equivalent. And I also added to my former stores, Anomodon viticulosum in fructification, Hypnum curvatum, triquetrum and velutinum, Verrucaria gemmata and Adoxa moschatellina. The vicinity of the castle was reached by noon, and at the base of the rock on which it stands Hypnum atro-virens was picked, and a specimen or two of Equisetum

Mackaii? close to the water; but E. hyemale, which grows here abundantly, was all cropped. Further down the Den, among overhanging rocks, Bryum androgynum was plentiful with little heads of gemmæ, and Tetraphis pellucida bearing gemmiferous cups, and at its foot, on a marshy bank, Chrysosplenium alternifolium displayed its curious flowers. Well loaded, I for this time bade adieu to Airly, and after a long smart walk reached Newtyle in good time for the last train.

My second visit to the Linn was on June 24th, when summer was glowing in all her loveliness. I followed the same route as formerly, and in shallow pools on the "market-moor" of Alyth collected fine specimens of Pilularia globulifera. The hedge-rows between Meigle and Alyth were bright with wild roses that perfumed the air, and abundance of Galium cruciatum shot up through their verdure: and upon the hills beyond Alyth, Gymnadenia conopsea and Polygonum viviparum were in luxuriance. The "public" was reached in time for tea, and I botanized about the Linn till nearly 10 o'clock. The same roar of rushing waters greeted my ear as before; but instead of the leafless trees, I now walked under a green umbrageous canopy, through which swelled at intervals the deep-toned music of the ebon bird of evening. My spring hope was now realized, for every crack and crevice of the rocks, every nook and crannie, bank and brae, was loaded with herbage and flowers. Galium boreale was abundant, and Meum Athamanticum I never gathered in finer condition. Solidago Virgaurea, Crepis paludosa and Hieracium murorum var. γ. *Lawsoni*, reared their golden blossoms from many a fissure in the cliffs, and the silvery foliage of Alchemilla alpina adorned the rocks. Geranium pratense was plentiful, and with Carduus heterophyllus, and the glowing flowers of Lychnis diurna, gave a flush of beauty and brightness to many a spot. Valeriana sylvestris, Stellaria holostea, and a host of other common though beautiful things, crowded on the attention from every side. The cryptogamic tribes were almost smothered by the proud ambition of the higher orders of vegetation, but I still was enabled to add several to my former list, among which the more interesting were Peltidea venosa, Nephroma resupinata and Trichostomum polyphyllum.

On the succeeding morning I gathered on the northern banks a profusion of Orobus sylvaticus and Melampyrum sylvaticum, and further down the Den, Melica nutans and uniflora, Carduus heterophyllus &c. But the stormy threatenings of the morning were soon followed by a heavy rain, which continued almost without intermission during the remainder of the day, and a thorough drenching to the skin tended in some degree to cool down my botanical enthusiasm. I searched about, as well as I could under the circumstances, for Orobus niger, but without success, and in vain looked for the "wet rocks above the castle," where the Timmia grows. In fact, by the time the foot of the Den was reached, I was as wet as if I had come down the Reeky Linn and swam the Isla all the way. But my vasculum had been well filled, and moreover I had the consolation of having chivalrously suffered in a noble cause,—even in the cause of the bright and beautiful Lady Flora. WILLIAM GARDINER.

Dundee, February 7, 1844.

ART. CCIII.—*Varieties.*

447. *Method of arranging the Cryptogamia in a Herbarium.* Many excellent methods of collecting and arranging herbaria have appeared in the pages of 'The Phytologist,' but as these all relate to the Phænogamous plants, I trust the following hints respecting the Cryptogamic tribes — Musci, Hepaticæ and Lichenes, may prove interesting to some of your readers. The more general mode of forming and arranging herbaria for mosses &c. seems to be that of glueing the specimens to the paper, taking care that this shall be of a sufficient transparency and whiteness, that the plant may be examined on holding it up to the light. Now this method is attended with much inconvenience; the different parts of these tribes are extremely minute, and require very careful investigation, the previous application of water is also absolutely necessary to most or all species. Can this be done to a specimen fastened on the paper, without detriment to its texture; and supposing water to be applied, may not the microscope disclose perhaps the most essential parts buried in a mass of glue? The remedy is very simple. Take half a sheet of stout foolscap, and divide this into quarters, which will be suited for the larger mosses. Again, these quarters halved will form another very convenient size. The half sheets divided into thirds, and these halved, will constitute a most useful size for the genera Hypnum, Bryum, &c. Other forms may be made according to the taste and wants of the collector. Now fold each of these divisions lengthways, so that the under side shall appear three-eighths of an inch beyond the upper: turn this border upon the upper edge, and do likewise with the two ends of the folded paper, you will then have a case somewhat similar to those in which a nurseryman encloses seeds for sale. These cases are to be glued to

paper formed by quartering half a sheet of foolscap, and on each quarter can be laid three or more of the cases (except those of the largest size, two of which will fill the paper). The species may now be distributed according to their respective sizes, and one paper will be allotted to each species, thus leaving a reserve for varieties, and specimens from different localities. The names will be written on each of the cases, and the whole formed into fasciculi, with the generic names outside; two or more fasciculi may be allowed for the more numerous genera. A herbarium formed according to this method is always neat, and easily referred to; the specimens may be taken out of the packets, examined and replaced without incurring any risk of injury. Extra papers can always be added when required. Very little trouble is necessary for drying mosses, Hepaticæ, &c. Specimens should generally be moistened previous to the application of pressure. Old books will be found a convenient substitute for loose paper: the specimens should be placed between the leaves and left untouched till thoroughly dry, nor need they be removed except when required. As directions are always much more easily written than understood, and fearing this might be applicable to the above, I gave them to a lady, requesting that she would put them in practice; and in a very short time, to my great satisfaction, I beheld many papers arranged exactly, though perhaps with greater neatness, like those in my own herbarium. — *F. Townsend; Ilmington, Shipston-on-Stour, January* 16, 1844.

448. *Notice of a Carduus found near Saffron Walden, Essex.* The following is a brief description of a curious variety of Carduus (arvensis?) found in the autumn of 1843, about four miles from Saffron Walden; it was growing by the road-side, on a poor clay soil, in the neighbourhood of a wood. Only two plants of it were in flower, one of which was spreading and much branched, the other small, but presenting similar characteristics. Near them C. arvensis, acaulis and lanceolatus were growing abundantly. Root creeping: stems much branched, about two feet high, leafy and woolly: leaves nearly similar to those of C. arvensis: flowers numerous, on separate stalks, as large as those of C. acaulis, and very similar in colour; calyx-scales mostly green; pappus rather short. In general habit it agrees most nearly with C. arvensis, but differs in its spreading, woolly stem, in its flowers being thrice the size, and of a crimson colour; in the calyx-scales being less coloured, and in the shortness of the pappus. From C. acaulis it differs in its size, *bushy character*, and in its *leaves;* also in its numerous flowers; but agrees in the colour and size of its

flowers and its woolly stem. Specimens of it have been shown to several eminent botanists, who have expressed very different sentiments respecting it, but most of them have considered it an interesting plant, and a form which they had never before seen. One is of opinion that it is an instance of Carduus arvensis being diœcious, this plant having the stamens more largely developed, which might account for the difference in the length of the pappus. Another considers that it has nothing to do with C. arvensis, but is C. acaulis, β. *dubius* of Willdenow's 'Species Plantarum,' and C. caulescens of Persoon. A third would give it the name of acaulis, had not the stems been of such a height, although he thinks the leaves more like arvensis. A fourth is ready to suppose, that it must be a hybrid between C. acaulis and arvensis, the flowers and hairiness of the stem bringing it exactly to the former, and the leaves and size agreeing with the latter. Several others have expressed opinions similar to one or other of those given above: and when such a difference exists among the most competent judges, I feel unwilling to hazard a sentiment, but the first explanation does not appear by any means to account for the whole of the very striking differences between this plant and C. arvensis; with the second I do not feel able to unite, because although C. acaulis has been frequently found caulescent in this neighbourhood, and I have a specimen of it gathered last autumn, with a stem *thirteen* inches in height, yet it always preserves, as far as I have seen, its general distinctive characters, and presents a very different appearance to the plant under consideration. I fear that there are not sufficient distinctive peculiarities to constitute it a separate species, and therefore the only other alternative seems to be to consider it a hybrid between C. arvensis and acaulis, which, though an uncommon occurrence in nature, is by no means an impossible one. Perhaps some of your correspondents may have seen a similar plant, and be able to decide the question, or some clearer light may be obtained after closer observation of it next season.—*G. S. Gibson; Saffron Walden, January*, 1843.

449. *On the dispersion of the Seeds of Oxalis Acetosella.* I think it is not generally known that Oxalis Acetosella has the power of projecting its ripe seeds to a considerable distance from the plant. Many of our native plants possess a similar power, such as Impatiens, Cardamine, Ervum, Ulex, and other Leguminosæ: in all these the expulsion of the seeds is effected by the rapid opening and curling up of the valves of the capsule; in the Leguminosæ the two valves become spirally twisted, in Cardamine they form a flat coil. In Oxalis,

however, the seeds are ejected, not by any movement of the outer valves of the capsule, but by a curious internal bag in which each seed is enveloped. The capsule consists of five cells, each containing one seed, and having an opening at the side. Each seed is enclosed in a closely fitting semitransparent skin; when ripe this little skin acquires the projecting power, having a strong inclination to turn inside outwards, which, continually increasing, ultimately causes a small fissure at the top, and immediately this is effected, the bag rapidly inverts itself, and the seed is thrown out *with a crack* through the opening provided for it; and sometimes the bag itself is thrown out by the violence of the movement, but generally it remains in the mouth of the cell.—*Joseph Sidebotham; Manchester, January* 10, 1844.

450. *Note on Veronica triphyllos.* While examining lately one or two very fine specimens of Veronica triphyllos, from the neighbourhood of York, with which I was favoured by my friend Sylvanus Thompson, Esq., I was struck with the great discrepancy between Hooker's description and these specimens. Hooker arranges this plant in the last section of the genus, having the "flowers axillary—solitary," whereas my specimens have the flowers in *terminal racemes*, exactly as in V. alpina, serpyllifolia, and others of the first division of the genus. Hooker indeed says of V. triphyllos,—"flowers subracemose," so that its being placed in a section having axillary solitary flowers, may arise from inadvertence. I may remark that the only other specimen of V. triphyllos in my collection, and which I owe to the kindness of Sir W. Hooker himself, shows exactly the same mode of inflorescence as in the Yorkshire specimens above alluded to. I quote the third edition of the 'British Flora,' perhaps the error may be corrected in a subsequent edition, to which at present I have no access.—*Thos. Edmonston, jun.; Baltasound, Shetland, January* 27, 1844.

451. *Villarsia nymphæoides.* As I am not acquainted with any recorded Scotch habitats of this beautiful plant, it may perhaps interest your readers to know that I gathered it in August, 1840, in a large mill-pond near Glasgow, where it was first observed by Mr. W. R. Murray of Edinburgh. The plant grows in great abundance in this station, intermixed with Potamogeton, Myriophyllum, and other aquatic plants. I regret I cannot give a more explicit description of this station, from my ignorance of the localities round Glasgow, and it was so dark when we gathered the plant, that we both took only a specimen a-piece as Nuphar lutea; and thus we did not pay that attention to the station that we should otherwise have done. The plant

has every appearance of being as indigenous as any of the plants among which it grows. I rather think the pond in which it grows was to the westward of the town; it was large and deep, and the Villarsia could only be reached in a boat.—*Id.*

452. *Carex boenninghausiana.* I inclose you a specimen of the Crichton-castle Carex alluded to in my List of additional Edinburgh Plants (Phytol. 407), and I think you will agree with me in considering it C. remota. Since reading the remarks by Mr. Luxford (Id. 650) and Mr. Gibson (Id. 779), I have carefully examined my numerous specimens from Crichton-castle, gathered at different times, and feel quite convinced of the correctness of my former opinion, that all I had met with in that station were C. remota, and none agreeing with the description of C. boenninghausiana: some of the Edinburgh botanists were indeed of opinion that my specimens were really C. axillaris, and I was inclined to think the same, or rather, that C. remota and C. axillaris were undistinguishable; for I could perceive little difference between the Crichton-castle plants and what I conceived to be authentic specimens of C. axillaris in my herbarium. Shortly afterwards, however, I became acquainted with the *true* C. axillaris, specimens of which I owe to the kindness of my valued correspondent, Dr. Wood, when I at once perceived that they were different both from the Crichton-castle and all other specimens called C. axillaris in my collection; and consequently, specimens from eight stations had, to my great disappointment, to be handed over to the C. remota sheet. Thus Dr. Wood's specimens stand in my herbarium as the sole representatives of C. axillaris, and to these I trusted in comparing my Edinburgh specimens, and in referring them to C. remota. I only know C. boenninghausiana by Mr. Luxford's and Mr. Gibson's notices, as no work I have access to at present contains a description of it. I would therefore be much obliged if you would examine the specimens now sent, and report. I make these remarks as I think C. boenninghausiana must be very rare in the Crichton-castle station, for I very carefully examined all the Carices I could find near the place in hunting after C. axillaris, but never found anything nearer it than the plant now sent.—*Id.*

[The specimens sent by Mr. Edmonston are all referrible to Carex remota.—*Ed.*]

453. *Note on Cetraria sepincola.* A few days ago I found some specimens of Cetraria sepincola near the west side of the island (Unst). I had not previously seen it in Shetland, and indeed it is not, I believe, a common plant anywhere in Britain. The specimens were rather small and without apothecia: they were accompanied by Ra-

malina scopulorum and farinacea and other lichens. Hooker, in his British Flora, says that he has never found Cetraria sepincola but on wood, but these specimens were found on a block of mica-slate.—*Id.*

454. *Remarks on Dr. Ayres' opinion of the Vaucheriæ.* Allow me a few words on the Vaucheriæ. Your correspondent Dr. Ayres (Phytol. 743) seems to wish to annihilate the genus, principally, it would appear, because Vaucher refers his Ectosperma (Vaucheria) terrestris to Protonema velutinum (*Agdh.*), and which latter is now believed to be a rudimentary moss. I am afraid our Algologists will scarcely consent to this summary mode of dealing with the genus Vaucheria, and I think Dr. Ayres can scarcely be acquainted with the V. terrestris of our botanists, or he would not follow Vaucher in confounding it with Protonema velutinum. That the whole genus Protonema is composed of the commencement of other Cryptogamia, chiefly mosses, is, I think, generally agreed on by botanists; but that "the Vaucheriæ are the rudimentary states of the mosses, and that the ovoid vesicles are analogous to granules, and reproduce the primordial state of the moss," is surely an untenable position. What a splendid moss must it be, whose byssoid commencement is one or two feet long, and as stout as a Chara! —yet if Dr. Ayres' views are correct, such is V. dichotoma: and where have the *salt-water* mosses come from to produce V. marina and V. velutina? If Vaucheria is denied a place as a separate genus, so I suppose must Bryopsis and Codium, for these genera are very closely allied to Vaucheria, indeed it is very difficult to separate the latter most singular genus from it by a definite character. As I am on the subject of these Cryptogamia, permit me a word on the plants associated with the supposed genus Protonema in the group Byssoideæ of the Confervoideæ,—*Hooker &c.* (Chlorospermeæ, *Harvey*); these consist of the genera Byssocladium, Mycinema, Chroolepus, Trentepohlia, Protonema, Hygrocrocis and Leptomitus. The whole genus Mycinema appears evidently to be altogether composed of imperfect Fungi, probably Thelephoræ, and Dr. Ayres and others refer M. phosphoreum to a not uncommon state of Auricularia (Thelephora) cærulea. If Chroolepus be retained in the Algæ, Monilia, Helmisporium and many other genera must be removed thither from the Fungi; surely however their structure is completely that of the latter tribe, and Arnott, Carmichael, Hooker, and other eminent Cryptogamists, are of this opinion. It seems to me likely that the Lichen heterophyllus of Smith (Eng. Bot. 2246), a plant which has occasioned considerable perplexity, and which is doubtfully referred in the 'British Flora' to Cornicularia (*Ach.*), is a species of Chroole-

pus or an allied genus. Hygrocrocis and Byssocladium should surely be classed near the Mucors: Leptomitus clavatus cannot, by its habitat (dead animals), be an Alga, (Br. Fl. 385): L. minutissimus is stated to be the fibrillæ so often found besetting the apices of the Polysiphonæ, (Brit. Flor.): L. lacteus (Conferva lactea, *Dillwyn*) seems a doubtful plant, but if an Alga, would surely rank better with the Gloiocladæ, perhaps near Chætophora or Rivularia. How Trentepohlia is arranged in this tribe, I cannot understand; its affinity is surely with Calithamnion, Griffithsia, &c. among the Ceramieæ, indeed I cannot distinguish authentic specimens of T. purpurea from Calithamnion Rothii; and Hooker says "I am almost inclined to think it may be that species altered by growing in situations where it is only occasionally wet with salt water." I have thrown out these hints in the hope of eliciting some opinion from better Cryptogamists than I can pretend to be.—*Id.*

455. *Note on Equisetum umbrosum.* With respect to your correspondent Mr. Gibson's enquiry (Phytol. 680), whether Equisetum umbrosum or E. arvense "be the more glaucous of the two," I may remark, having gathered both plants, that Francis is correct in saying that the former is the more glaucous plant. The passage referred to in the 'British Flora' seems likely to be an error of the press, for the light green glaucous colour of E. umbrosum is a very apparent and remarkable character, and which distinguishes it, even at a distance, from any other British Equisetum that I am acquainted with.—*Id.*

456. *Note on Hieracium hypochœroides, in reference to Mr. Watson's Notes.** I now give the following descriptions of Hieracium murorum, pulmonarium and maculatum, from Smith's 'English Flora.'

> " *H. murorum.* Stem corymbose, with a solitary leaf. Leaves ovate-heart-shaped, wavy, with radiating teeth chiefly at the base."—Eng. Flora, iii. 359.
>
> " *H. pulmonarium.* Stem somewhat corymbose, solid, slightly leafy. Leaves lanceolate, deeply and unequally toothed throughout; teeth pointing forward."—Id. 360.
>
> " *H. maculatum.* Stem cymose, many-leaved, tubular. Leaves ovate-lanceolate, strongly toothed; teeth pointing forward."—Id. 362.

It will be evident on examination that the descriptions above quoted do present characteristics sufficiently distinct to justify the adoption of three species, and I will now see how far my plant will agree with those descriptions. On examination I find my plant to differ from the description of H. murorum, in the stem being only *one-* or *two-flowered* (Mr. Watson says it has from *one* to *three*); this cannot be considered a corymbose stem: it also differs in having the teeth of its leaves pointing forward, and therefore cannot be said to have radiant teeth. The want of a corymbus and *lanceolate* leaves will serve to distinguish it from H. pulmonarium. Its *naked* and *solid* stem will serve to distinguish it from every form of H. maculatum. Many other characters might have been pointed out to distinguish the above plants, but as Mr. Watson only refers to Smith, I will seek no other authority and will now conclude with saying that I am much surprised to find that Mr. Watson, after he has paid so much attention to Botany, should consider the absence or presence of medulla in the stems of plants, to be of no value as a character whereby to distinguish them as species. It is to be regretted that such valuable characters are not duly appreciated; but it is an indisputable fact, that unless we submit to a careful investigation, the greatest errors may be committed. *Samuel Gibson; Hebden Bridge, February* 10, 1844.

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* The former part of Mr. Gibson's communication is merely a repetition of what Mr. Watson has written at pp. 801 and 841, to which we beg to refer the reader. *Ed.*

457. *Enquiry respecting Phascum multicapsulare*, Smith. Permit me to enquire, through the medium of 'The Phytologist,' whether anything is known concerning this moss, which is in all probability distinct from every other described British species. It has usually been regarded as a variety of P. crispum, (see Hooker and Taylor's 'Muscologia Britannica' &c.); but it appears from the account given of it in Bridel's 'Bryologia Universa,' i. 48, that Dr. Mohr, having received specimens from Mr. Turner, saw reason to abandon such an opinion; and accordingly, in Weber and Mohr's Bot. Tasch. p. 477, it is separated from Phascum crispum, on the ground that it has the *leaves evidently serrulate;* and Smith (Flor. Brit. 1152) describes it as differing from P. crispum in having its perichætial leaves *straight, and never crisped when dry;* also of a more dusky hue. The place of growth is stated to be in cart-ruts in Clapham-park wood, near Clapham-ford end, and other places near Bedford, (Rev. Dr. Abbott). It is, however, figured under the name of Phascum crispum in 'English Botany,' t. 618, and at that time Smith entertained no doubt of its being the true Hedwigian species so called. A subsequent change of opinion on the part of Smith, is a strong argument in favour of the moss being distinct; and it is hoped that some one residing in Bedfordshire will endeavour to serve the cause of science in instituting a search for this moss. The numerous capsules on each stem will serve to indicate the plant. — *W. Wilson; Warrington, February* 20, 1844.

458. *Note on the production of Shoots of Cytisus Laburnum and C. purpureus from grafts of C. Laburnum coccineum.* In Mr. Young's

nursery-ground at Milford, near Godalming, there are several trees of Cytisus Laburnum coccineum, similar to that in the Royal Botanic Garden at Edinburgh, of which specimens were exhibited at the meeting of the Botanical Society of Edinburgh, on the 8th of June, 1843 (Phytol. 653), in which, from the grafting of the French hybrid—the C. Laburnum coccineum, on the C. Laburnum, the result has been the production of shoots of three varieties,—C. Laburnum coccineum, C. Laburnum and C. purpureus. I observed these trees in blossom last year, when, as in the Edinburgh tree, the yellow and red flowers were predominant. Even in their present leafless condition they present a singular appearance: the cherry-tree-like branches of C. Laburnum coccineum being very distinct from those of C. Laburnum, which retain their decayed seed-vessels, while the pendulous tufts of C. purpureus appear, at a short distance, like bunches of mistletoe. Mr. Young states that it is about six years since he first noticed the tendency of the mule laburnum to reproduce the forms of the parent plants.—*Henry Bull; Godalming, Surrey, February* 19, 1844.

459. *Republication of Johnson's 'Itinera Botanica.'* We are pleased to observe that Mr. Pamplin is about to republish this interesting work by subscription. The text will be translated verbatim by Mr. A. Irvine.—*Ed.*

460. *Collections of Scottish Plants.* Mr. Wm. Gardiner has announced his intention of preparing, during the ensuing summer, a number of packages of plants, chiefly from the alpine district of Braemar. Subscribers' names received by Mr. Gardiner, Dundee.—*Ed.*

ART. CCIV.—*Proceedings of Societies.*

LINNEAN SOCIETY OF LONDON.

November 7, 1843.—R. Brown, Esq., V. P., in the chair.

Dr. Bromfield, F.L.S., presented a specimen of a species of Calamintha found by him in the Isle of Wight, and regarded as new, (see Phytol. 768).

Mr. Newman exhibited a specimen of Trichomanes lately found in the co. Kerry, and supposed to be distinct from T. speciosum.

Read, a letter from Joshua Clarke, Esq., of Saffron Waldon, accompanying specimens of Barkhausia setosa (*Dec.*), found in that neighbourhood, with a note on the characters and distribution of the species, by Mr. Kippist, Libr. L.S.

Read also the commencement of 'An Analysis of Rhizantheæ,' by Wm. Griffith, Esq., F.L.S.

November 21.—E. Forster, Esq., V.P., in the chair.

Read the conclusion of Mr. Griffith's 'Analysis of Rhizantheæ,' and his 'Description of Sapria, a Himalayan genus related to Rafflesia.'

December 5.—E. Forster, Esq., V.P., in the chair.

Read, 'Observations on Cytineæ, and on the genus Thottea of Rottböll,' in continuation of Mr. Griffith's memoirs on Root-parasites.

December 19.—E. Forster, Esq., V.P., in the chair.

Mr. J. T. Lay, Her Majesty's Consul at Canton, presented a box of specimens of the *Keih-seen-me*, a species of Alga related to Nostoc, and eaten as a delicacy among the Chinese.

Read, a paper 'On Carex saxatilis (*L.*), and an allied species,' by Francis Boott, M.D., F.L.S. &c.

The allied species was found in 1832, in Glen Phee, Ciova, by the party accompanying Dr. Graham on his annual botanical excursion to the Highlands, and was considered a form of C. saxatilis, *L.*; but Dr. Boott's attention having been called to it by Mr. W. Wilson, he has been led to regard it as a distinct species, and has named it C. Grahami. The following characters are given: —

Carex Grahami. Spikes 4—5, cylindrical, ferruginous; barren spikes 2 (rarely 1), slender, acute; fertile spikes 2—3, rather remote, stout, obtuse, the lower ones pedunculated, without sheaths, somewhat nodding: stigmas 2: perigynium oblong-ovate, inflated, nerved, somewhat erect, ferruginous (rarely straw-coloured), pale at the base, twice the length of the glume, beak bifurcate; glumes ovate, acute, brown, white at the apex, with a pale nerve.

Carex saxatilis. Spikes 2—3 blackish purple; barren spike 1 (rarely 2), cylindrical, pedunculated; fertile spikes 1—2, rounded or ovate, the lower one more or less pedunculated, bracteated, without a sheath, erect: stigmas 2—3: perigynium somewhat globose or ovate, with a notched beak, stipitate, spreading, nerveless, blackish purple, paler at the base, longer than the glume; glumes ovate, rather obtuse, blackish purple, white at the apex, nerve concolorous. C. saxatilis, *L. Fl. Lapp.* 259 (1737). C. pulla, *Good. in Linn. Trans.* iii. t. 14 (1795). Hab. Alpine districts of Scotland, Norway, Lapland, Sweden, Iceland, and the Faroe Isles.

The author critically examines the original authorities which show the C. pulla of Goodenough to be identical with C. saxatilis, *L.*; he also points out the origin of the confusion of the latter with C. rigida, *Good.* The characters distinguishing C. Grahami and C. saxatilis are then more minutely examined, and the author adds that he should not doubt their specific distinctness, but for the observations of Drejer, who, in his 'Revisio critica Caricum Borealium,' under the name of C. pulla, *β. fusca*, describes specimens from Iceland and Greenland closely agreeing with C. Grahami, except that no mention is made of the nerves of the perigynium, and remarks that the Greenland specimens are so very variable that they would scarcely be thought to belong to the same species. In the absence of precise information on the subject, Dr. Boott is inclined to refer to Presl's C. physocarpa (a native of Nootka Sound), both the larger Greenland specimens, and others from the Rocky Mountains described by him as C. saxatilis in Hooker's 'Flora Boreali-Americana.' The author, while he repeats that both himself and Mr. Wilson consider C. Grahami entitled to rank as a species, yet leaves it to future observers to determine the value of the character given for it, and whether it is to be retained as distinct, referred back to C. saxatilis (*L.*), or transferred to C. physocarpa (*Presl.*)

Read also, 'An account of the Trees producing Myrrh and Frankincense, as found in those parts of the coast of the Red Sea and Indian Ocean whence those Gums were obtained in the first dawn of Commerce;' by Major W. C. Harris, late on an Embassy to the Court of Shoa, in southern Abyssinia.

BOTANICAL SOCIETY OF LONDON.

February 2, 1844.—A. Gerard, Esq., in the chair.

Various donations to the library and herbarium were announced, including 44 new species of mosses, collected at Swan River by Mr. James Drummond.

Read the commencement of a paper by Edwin Lees, Esq., F.L.S., being 'A Synoptical View of the British Fruticose Rubi, arranged in Groups, with explanatory remarks.' The groups into which Mr. Lees unites the species have been already reported (Phytol. 655). The list of species will shortly be published in a new Catalogue of British Plants, now in the press for the Botanical Society of London.

The following explanations, in the words of the author, will sufficiently show that this arrangement has not been founded upon any brief or superficial study of his subject.

"Having previously designated the general groups into which the British Fruticose Rubi are divisible, I now proceed to attempt the more difficult task of describing the species in each group, and tracing them in succession in a synoptical form. In doing this, as I must necessarily propose some alterations, it is advisable that the candid and enquiring botanist should be informed as to the principles I have kept in view.

"In the first place, then, I have desired to make no innovation but what seemed imperatively required for correct elucidation, and have therefore made every effort to profit by the labours of preceding eminent botanists, who have particularly studied the Rubi, as Sir J. E. Smith, Drs. Weihe and Nees ab Essenbeck, Mr. Borrer and Professor Lindley. But secondly, I have observed with the eye of an original explorer, tracing every form that appeared to me different, without reference to the ideas of other botanists. And thirdly, having observed the same plants in a living state for several successive years, I have collated and revised my original observations, sketched every apparent species, and compared them again and again with the figures, descriptions and named specimens of botanists of authority. Thus I have been enabled in a great degree to understand the forms to which particular names have been assigned, and to test their propriety by my own experience. I trust, therefore, that I shall not be considered guilty of assumption when I may differ from others, being only anxious for the nearest approximation to correctness.

"It is unnecessary for me to go into the question as to what constitutes a species in this genus; for, as I have before hinted, it is not unlikely that the forms in every *group* may be really only varieties, sporting from a normal form and into each other. But if Botany be a science of discrimination, it is at any rate convenient to name every remarkable continuing form as a species or sub-species, since otherwise minor variations can scarcely be distinguished, or must be placed in the same rank with more important deviations of structure. Indeed Nees Von Essenbeck, one of the authors of the elaborate 'Rubi Germanici,' has well remarked in a letter to the Rev. Mr. Leighton, in the Shropshire Flora, — 'I am not of opinion that all the forms proposed by my friend Mr. Weihe *as species*, are to be considered as such, but in my opinion it is absolutely necessary to look for the greatest number of forms which present themselves in the genus, before attempting to judge of species and fixing their limits. I can scarcely tell which is most perplexing in the path of our science, whether, with Mr. Weihe, to distinguish as species every form of bramble that presents itself to our view, or, with Mr. Koch, to consider all as modifications of one only. In this case I do not doubt that these are matters purely of observation, and that the faithful observer of Nature will find that the truth really is between these two extremes.'" *G.E.D.*

Errata.—Page 880, line 31, for *cautecaulis* read *caulicola:* line 32, for *Hystericum* read *Hysterium.*

Page 889, line 4, for *reticulate* read *parallel veins:* line 5, for *parallel veins* read *reticulate.*